

Fitz - Gibbon (m)

AN

EXPERIMENTAL AND ANALYTICAL

ESSAY,

ON

THE POWERS

BY WHICH

THE BLOOD IS CIRCULATED

IN

THE VEINS.

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BY MAURICE FITZ GIBBON, M. D.

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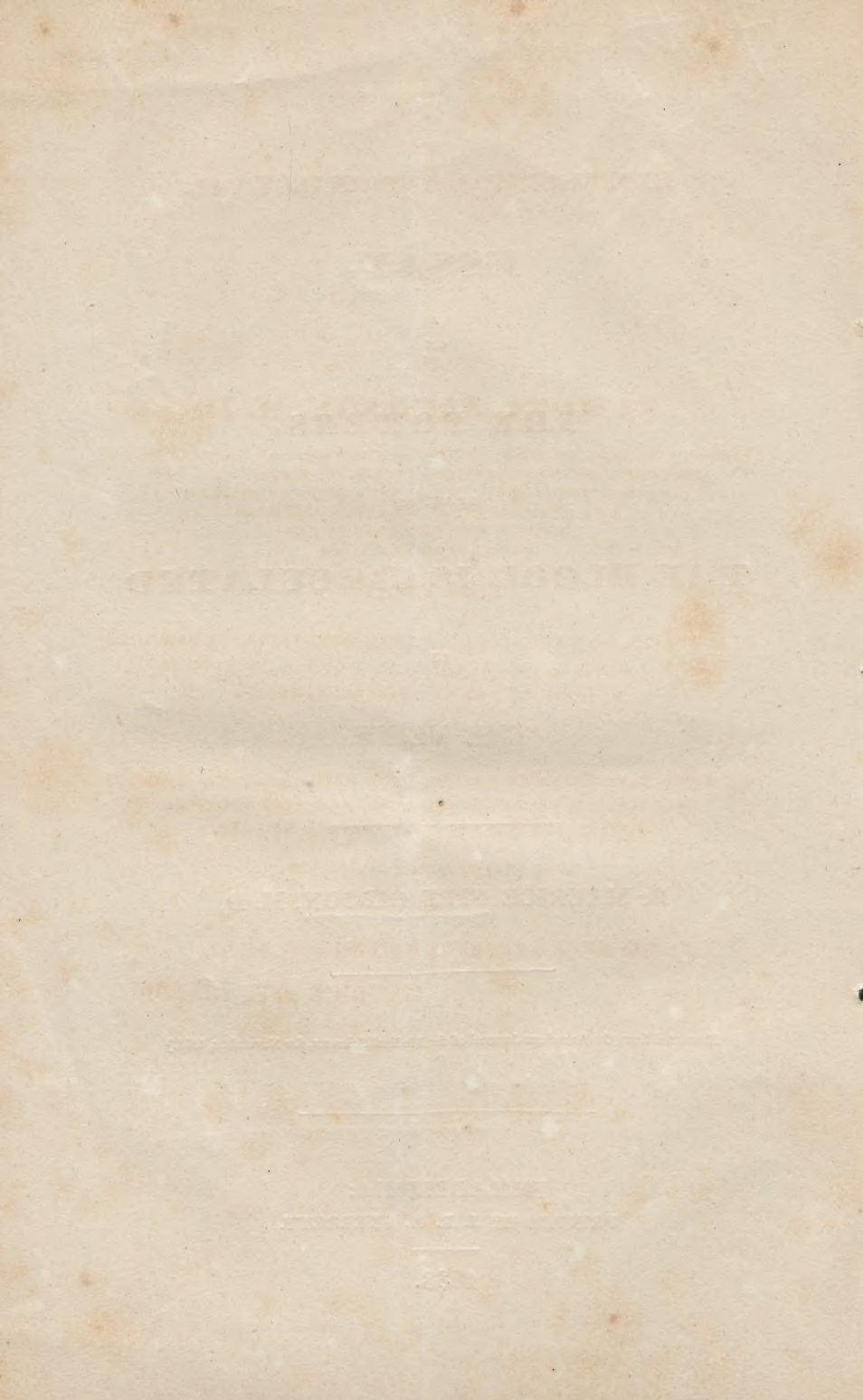
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TO
SAMUEL JACKSON, M. D.

PROFESSOR OF THE INSTITUTES OF MEDICINE AND CLINICAL MEDICINE IN THE
UNIVERSITY OF PENNSYLVANIA; LECTURER ON THERAPEUTICS AND
MATERIA MEDICA IN THE PHILADELPHIA MEDICAL INSTITUTE;
VICE PRESIDENT OF THE PHILADELPHIA MEDICAL
SOCIETY; VICE PRESIDENT OF THE
COLLEGE OF PHARMACY, &c.

THIS ESSAY,
THE FRUITS OF PHYSIOLOGICAL INVESTIGATIONS, COMMENCED
BY HIS ADVICE, AND PURSUED IN A SPIRIT, TEMPERED,
GUIDED, AND CHERISHED BY HIMSELF,

IS,

WITH THE SENTIMENTS OF THE HIGHEST REGARD FOR HIS EMINENT
TALENTS AND PROFESSIONAL ZEAL, AND OF UNFEIGNED
ADMIRATION FOR HIS MANY PRIVATE VIRTUES,

MOST RESPECTFULLY DEDICATED,

BY

HIS EVER GRATEFUL FRIEND AND PUPIL,

MAURICE FITZ GIBBON.

WILLIAM L. JACKSON, M.D.

TO THE HONORABLE SENATE AND HOUSE OF REPRESENTATIVES
OF THE UNITED STATES OF AMERICA
IN SENATE AND HOUSE CHAMBERS
WASHINGTON, D.C.

THIS DAY

PRESENTED BY MR. JACKSON, MEMBER OF THE HOUSE OF REPRESENTATIVES
IN SENATE AND HOUSE CHAMBERS
WASHINGTON, D.C.

WITH THE BEST WISHES OF THE SENATE AND HOUSE OF REPRESENTATIVES
OF THE UNITED STATES OF AMERICA
WASHINGTON, D.C.

THE HOUSE OF REPRESENTATIVES

THE HOUSE OF REPRESENTATIVES

MAURICE R. GIBSON

ESSAY, &c.

THE powers which circulate the blood, have been subjects of inquiry ever since the great discovery of the circulation, and numerous are the opinions that have been offered to account for the motion of this fluid; and yet it may be fairly asked, what do we really know, on which there rests no doubt in addition to these facts, that the blood is circulated, and that it acquires motion by the contraction of the heart. It is true we do know a little more, but this little has been, I think, greatly overrated, both in respect to its importance, and the degree of certainty which is attached to it.

Notwithstanding all that has been said on the subject of the diastole of the cavities of the heart, it is not yet generally understood, whether the fibres of the heart are active or passive while the diastole is being effected, i. e. whether the fibres constituting the muscular parietes of the auricles and ventricles of the heart, are relaxed in each cavity during the time in which they receive blood, or whether they are actively concerned in dilating the cavity into which the blood enters. The opinion of HARVEY is that which almost generally prevails in the schools. He contended that a state of inertia or relaxation succeeded to the systole of the heart, and that the heart itself is the sole mover of the blood. Thus supposing the auricles to be full of blood, and the ventricles empty; the former, according to his view, contract, force the blood into the latter cavities, separate their relaxed walls from each other, and distend them. The ventricles now filled with blood, contract; the tricuspid and mitral valves are closed by the pressure made on the blood; the semilunar valves are opened by the same cause; the blood is forced into the arteries of the greater and lesser circulation, it moves onwards through the arteries into the veins, back again through the latter vessels to the auricles of the heart with undiminished velocity, and these cavities it enters, dilates, &c. The blood from the right ventricle goes through the arteries of the lungs, and from these into the pulmonary veins, thence into the left auricle. The left auricle forces it into the left ventricle, which contracts, forces it into the aorta, thence into all parts of the system, from which it is continued in motion by the force impressed upon it, into the veins, and by these to the right side of the heart.

Of all the modern writers who adopt the above opinion, there is no one who is less equivocal in his belief of it than Dr. BARRY. "The

supposition," he says, "that the cavities of the heart possess the power of dilating themselves, and therefore of acting alternately as suction and forcing pumps, although adopted by some physiologists, has hitherto derived but little support either from anatomy or experiment. This opinion was too trite, even in the days of Harvey, to merit serious refutation. Neither the auricle nor the ventricle appears to be furnished with any intelligible muscular apparatus, by which either can accomplish its own dilatation."

Although the opinion of those who contend for the active diastole of the heart is not incompatible with the peculiar doctrine of Dr. Barry on the "*influence of atmospheric pressure upon the progression of the blood in the veins*," still it seems to be any thing but pleasing in his sight, and creditable in his estimation. Were Dr. Barry to view the influence of the active diastole in the light I do, he might acknowledge that it is real, and yet not fear for the credit of his own views. But Dr. Barry, in denying the diastole to be active, denies, in fact, what he appears to aim at, that is, the suction power of the heart. It would appear, if we may be permitted for a moment to look into Dr. Barry's sentiments respecting his discovery, that he strongly apprehended a rival pump in the diastole of the heart, even though he assures us, "*the supposition that the cavities of the heart possess the power of dilating was too trite even in the days of Harvey, to merit serious refutation.*" Now I admit that Dr. Barry's views on the influence of respiration are not without proper foundation, but in my opinion he has greatly overrated the influence actually exerted by that power. Those also who have contended that the heart acts as a powerful sucking pump, have also, in my opinion, been equally in error with Dr. Barry; while the views of Harvey, though forcibly objected to by all who contend for the powerful influence of suction in one or the other way, appear liable but to a few objections. An inquiry into the influence of the agents to which I have now alluded, will constitute the principal subject of this paper.

Active diastole of the heart.—Experiment has proved in the hands of many, who have carefully investigated the subject, the active diastole of both the auricles and ventricles to be real, contrary to what Dr. Barry asserts; and we can oppose to his opinion on the subject, that of BICHAT, MAGENDIE, RICHERAND, LANGRISH, HAMBERGER, and several other eminent physiologists. Still more recently we have the authority of Dr. THOMAS ROBINSON, of Virginia, a highly talented physiologist, who published his remarks on the circulation of the blood in the 22d No. of this Journal.

Richerand, it must not be concealed, has used the following language:—"The cavities of the heart, however, are not *entirely* pas-

sive during dilatation;" and this leaves me somewhat at a loss how to consider his opinion on this subject, for I do not believe simply in a diastole, in the least extent passive, but fully and completely active. However, he either believed in the active diastole of the heart, or was most inconsistent with the results of his own observations, for he also says, "if you attempt to check the diastole of the heart, this organ resists the hand which compresses it, and its cavities appear endowed with a power which GALEN termed *pulsive*; in virtue of which, they dilate to receive the blood, and not because they receive it."

Magendie also observes—"all that has been said of the force of the heart, relates only to its contraction, its dilatation having been considered as a passive state, a sort of repose of the fibres; however, when the ventricles dilate, *it is with a very great force*, for example, capable of raising a weight of twenty pounds, as I have many times observed in animals recently dead. When the heart of a living animal is taken hold of by the hand, however small it may be, it is impossible by any effort to prevent the dilatation of the ventricles. *The dilatation of the heart then cannot be considered as a state of inaction or repose.*

Even Bichat, who lays a lower estimate on the influence of the heart in circulating the blood, than any other physiologist, says, "we shall see that this kind of motion is real to a certain extent, both in the heart and the organic vessels. But here it is wholly different; *the heart dilates of itself when it is empty, as we see by drawing it out of a living animal, and by emptying it afterwards of the fluid it contains, because it has in itself the cause of dilatation.*"

We do not find in the works of the older physiologists, opinions similar to those I have now given, for reasons I shall hereafter notice; but that Harvey, Haller, and others felt the heart dilate as forcibly as Bichat, Magendie, and Richerand, cannot be doubted; they however did not understand it alike with those who have entertained the opposite opinion.

I might go on to show, that the diastole is active, by many other authorities who have observed and described the action of the heart, were it really necessary. But I cannot omit referring to the case of Dr. Robinson, in a previous No. of this Journal, in which the active diastole is so fairly and satisfactorily observed, as to remove whatever doubt may still exist on the subject.

I should be guilty of an unpardonable omission were I at this time to leave unnoticed the view of Dr. HOPE, of London, "*of the physiology of the heart's action*," which he terms new in the very title page of his "Treatise on the Diseases of the Heart and Great Vessels," published in 1832. This view, notwithstanding the great pre-

caution which the author used to obtain correct results in his experiments, appears to us to be extremely erroneous, and calculated to misguide every person who may confide in the principles which he has laid down. While I thus openly express my sentiments as to the character of the work of Dr. Hope, I can with pleasure say, that I have no where met the action of the heart so faithfully, fully, and, in my opinion, correctly described. His experiments are minutely detailed, and I am satisfied every particular as it appeared is correctly recorded; but he did not permit his views to be governed by his experiments, and the exact observations which he took the trouble to make.

It is scarcely necessary to remark that the rythm, or order of succession in the action of the heart, is a matter of the highest importance to be understood, and this of course can only be the case when the action of the heart itself is distinctly known. According to the observations of Dr. Robinson, the following is the order of succession—diastole, systole, repose, and, as he observes, “Bichat’s experiments led him to the same conclusion,” and presently I shall show, from Dr. Hope’s account of his own experiments, that he has seen exactly the same thing with Dr. Robinson. The rythm, according to Dr. Hope, is systole, diastole, repose. I conceive the entire error of Dr. Hope to arise from the following opinion, with which he almost commences his book. Speaking of the diastole of the heart, he remarks, “it is perhaps safer for the present to attribute the diastole to that power by which a muscle reverts from the state of contraction to that of relaxation, and which I shall, for the sake of avoiding circumlocution, designate by the term *elasticity*.” Now, this diastole Dr. Hope terms active, but let it not by any means be understood that this diastole is the same as that which I contend for. Again, Dr. Hope says, “the systole is followed by a diastole, which is an instantaneous motion, accompanied with an influx of blood from the auricles.” I have now only given what we may consider as mere opinions of Dr. H. but in the following we have in his own words also an account of an experiment. “I found the auricle to contract first, not slowly, but with a motion so rapid as to be almost instantaneous; the moment the fluid reached the ventricle the latter was seen to *start up* evidently by the contraction of its fibres on the fluid which it contained, and *not by passive* dilatation. This more fully proved at a later period of the experiment, when the action of the heart was from time to time suspended, and the ventricle lay quiescent, though *partially distended with blood*, for then the auricle often made two or three contractions which had no stimulant effect on the ventricle, while a fourth, not more violent than the preceding, and therefore

not injecting more blood, caused it to spring up in the manner already described." Dr. Hope then unquestionably saw and remarked what I consider to be the active diastole. This very "starting up" of which he speaks, I regard as an active diastole, and such it must be regarded, unless we grossly misinterpret its character; his own words, in fact, are so plain on this subject, that they need no comment to show that he most assuredly saw the heart dilate, but as this probably did not agree with his preconceived notions, he did not see it to advantage.

Dr. Hope has seen the ventricle start up by the contraction of its fibres, "*and not by passive dilatation.*" Dr. Robinson felt the ventricles *suddenly spring dilated with surprising force;*" have these gentlemen, then, not seen and felt exactly the same thing? i. e. have they not observed an active diastole of the heart? Certainly they have.

Dr. Hope has also afforded us another evidence in favour of the active diastole of the heart; he says, "the ventricle lay quiescent though partially distended with blood, for then the auricle often made two or three contractions which had no stimulant effect on the ventricle; while a fourth, not more violent than the preceding, and therefore not injecting more fluid, caused it to spring up in the manner already described." Here, then, we perceive that without the power to dilate actively, the auricles could not distend or fully expand the ventricle. Mr. BRODIE, and many other physiologists have witnessed the same want of power in the auricles; but they did not recognise that a diastole and not a systole followed the contraction of the auricles. I have seen the auricles contract and dilate in animals as many as seven times for one of the ventricles when the lungs were collapsed, but when respiration was maintained by artificial means the ventricles recovered to a considerable extent their power of acting. "The fourth contraction," says Dr. H. "caused the ventricles to spring up." What an illogical conclusion is this! how easily refuted! as if, indeed, the power of the auricles were the cause of the dilatation of the ventricles.

Dr. Hope supposing that the diastole follows the systole, speaks of blood entering the ventricles:—"The systole is followed by a diastole, which is an instantaneous motion, accompanied with an influx of blood from the auricles by which the ventricles reexpand." This I know to be incorrect as a general rule; in some animals I have no doubt blood may enter the ventricles at the commencement of the repose from the auricles, but in my own experiments I have seen the ventricle of the frog's heart after contraction, small, pale, and void of blood, and suddenly it would "start up" and instantly receive blood and expel it again, and suddenly return to the state of repose

In contracting, the heart is thrown into a state from which it rebounds, or falls back again, as Dr. Robinson described it, to the "stillness of death," and although in the regular or natural frequency of action this is but a moment of repose, yet so perfectly at rest are the ventricles, that this time of repose, even though it be but for the third of a second, is so well marked and evident, that we are always struck with it in observing the action of the heart. What is meant by the "stillness of death" will be understood when the heart is observed just when it is removed from an animal after its life has disappeared, or in observing a heart in action, it will be perceived that the time of repose becomes longer as life declines. When an animal is struck on the head, and in this way made insensible, on opening its thorax and pericardium we find the heart in a state of rapid and violent action; in a short time it becomes slower and more regular in its movements, the times of repose become gradually longer, until finally the scene closes and all is at rest. The heart however undergoes no sudden change in its appearance. The repose in death resembles the last repose in life, except in its duration. On cutting through the ventricles of the heart soon after they have died, we find in some cavities of considerable size, in others the walls closely approximated. In the latter state I have found the hearts of birds, frogs, and some other animals; while in others again, the cavities were much larger, but capable of becoming much more capacious.

As the heart then, in many cases, presents the ventricles by no means dilated in the state of repose, and consequently containing but little or no blood in that state, it is evident, I think, that all who have described the diastole of the ventricles as preceding the state of repose, and succeeding the systole, have greatly erred. The ventricles unquestionably return to the state of repose with some force, and all the appearance of being contracted in that state is absent; the ventricles really appear at rest, and the form which they have then is not different from what they present soon after death: this is in my opinion a state of relaxation as complete as that which any other muscle of the body enjoys, but the appearance here is somewhat different, for the ventricles feel firm and rather unyielding. The ventricles then, in the state of repose, I do not believe to be under the influence of any power tending to dilate or contract them, and here I must differ from Dr. Robinson, who says "the antagonizing powers were merely in equilibrio," or that "probably the contracting power predominated." This opinion, it will be seen, quadrates perfectly and without any forced effort, with the result of my experiments on the structure of the heart.

The active diastole of the auricles, though less striking than the

diastole of the ventricles, is notwithstanding not less real. It is however not powerful like that of the ventricles, but it happens equally quick with it. The active diastole of the auricles did not escape the notice of Magendie, who remarks, "I have said that the blood of the three veins that are in the right auricle makes a considerable effort to penetrate into it. *If it is contracted* this effort has no effect, *but as soon as it dilates* the blood enters its cavity, fills it completely, and even distends the sides a little; it would immediately enter the ventricle if it did not contract itself at this instant. The blood, then, confines itself to filling up exactly the cavity of the auricle; but this very soon contracts, compresses the blood, which escapes into the place where there is least compression; now, it has only two issues—1st, by the vena cava; 2d, by the opening which conducts into the ventricle. The columns of blood which are coming to the auricle present a certain resistance to its passage into the cavæ or coronary veins. On the contrary, it finds every facility to enter the ventricle, since the latter dilates itself with force, tends to produce a vacuum, and consequently draws on the blood instead of repulsing it." The same is true for the opposite side of the heart. I have here quoted at length the opinion of Magendie, not only in relation to the diastole of the auricles, but on other points of much importance, because I am convinced, by repeated observations on the action of the heart, of their correctness.

In the 12th Vol. of the Philadelphia Journal of the Medical and Physical Sciences, (for 1826,) Dr. HAYS published his views on the forces by which the blood is circulated. On this subject he observes, that "in vain some physiologists have contended that the heart is dilated, and does not dilate itself, and that this dilatation is absolutely passive; the dilatation of the heart is a true effort, an active movement," &c.

Dr. Robinson says, "the diastole appeared to commence in the venous sinus, and pass without interruption and with immense velocity to the apex; the systole pursued the same course with equal velocity." There is then no alternation according to this gentleman in the action of the auricles and ventricles; they are in a state of repose at the same time, they dilate at the same time, they contract at the same time only with this difference, that action always commences in the auricles and extends into the ventricles without interruption."

The description of the action of the heart given by Magendie appears to me to be more in accordance with facts than that of Dr. Robinson. Dr. Hope's opinion on this subject is less exceptionable than Dr. R.'s. He has not represented phenomena as they appeared to him, out of their natural order, but he failed exceedingly, as I think

I have already shown, in not recognising them in their true character: he admits that the systole of the auricle is followed by the dilatation of the ventricle; but he erroneously considered the diastole thus produced as the consequence of the preceding auricular systole, and not what resulted from the "true effort," "the active movement" of the ventricle itself. He also mistook the repose of the ventricles for a state of distention; which indeed could not be called with propriety a state of repose; and the collapse of the heart after the systole, he mistook for an active diastole, and by this last error he was led into false principles. Dr. R. however, has not expressed his opinions without doubt, and if there be any room to object to his having ventured to institute principles, I think it must be, that he never extended his observations beyond the case which he has published. Leaving the views then of these gentlemen for the further examination of those who may be particularly interested in the improvement of auscultation, I now return to the subject with which I am at present more particularly interested, but here again I find myself opposed to the opinions of Dr. R. in a new point of view.

"While Bichat," says Dr. Robinson, "affirms that the heart dilates with a force which no effort of the hand could prevent, is it not surprising that he has neglected to supply a power so great and so obvious to the elucidation of that obscurity of the venous circulation, on which, he acknowledges, authors have hitherto shed few rays of light; to you it is unnecessary to demonstrate that the pressure of the atmosphere on the veins external to the cavities must be propagated to the termination; that the cavities being always full, there is a continued pressure on the vessels within them; add to this the powerful dilatation of the heart, auricles and ventricles coöperating in the action, as I have seen them, and you have all the requisites of a powerful sucking-pump, operating perpetually on the venous system."

Dr. Robinson's surprise that Bichat did not apply the active diastole of the heart to the "elucidation of the obscurity," &c. is easily accounted for. Bichat viewed the diastole of the heart in a manner very different indeed from Dr. R. Bichat did not believe that the auricles and ventricles coöperated in the dilatation of the heart; how then could he, indeed, look on the heart as a "powerful sucking-pump, operating perpetually on the venous system;" for to act as such, I contend the auricles and ventricles should dilate simultaneously; now this I also contend they never do,

I may still further notice the argument of Dr. Robinson in favour of his opinions, "founded on the structure of the heart itself." "Whoever," he argues, "inspects with candour and attention the structure of the valves, will find it difficult to persuade himself that

they are adequate to the function generally assigned them; he may easily ascertain that they offer an impediment to the reflux of the blood, but hardly that they afford a complete obstruction; but as the contraction of the auricles is less powerful than that of the ventricles, such reinforcement seems necessary to prevent reflux."

I have, nevertheless, examined the hearts of many animals with "candour and attention," and still I cannot persuade myself that they are not adequate to the function which the illustrious Harvey first assigned them, and which I am satisfied they will be regarded as executing by all who examine them.

The contraction of the auricles always aids in filling the dilating ventricles, and is, I think, the principal cause. I admit, of course, that atmospheric pressure would cause a fluid to rush through an opening into an exhausted vessel; but I cannot, on this account, see any good reason why indeed atmospheric pressure, and not the auricular contraction, should cause blood to pass from the auricles while contracting into the ventricles while dilating. The systole of the auricles is prompt and sufficiently powerful to throw blood into the ventricles; it commences an instant before the diastole of the ventricles, it is in full force when the latter commences; and when the systole of the ventricles is in operation, the auriculo-ventricular valves are closed, and the diastole of the auricles commences.

That atmospheric pressure is calculated also to cause blood to enter the auricles when they are dilating, is what cannot be denied; it certainly does not oppose the movement of the blood into the auricles, but if from other causes the blood move with sufficient velocity, as I think it does, why assign an effect entirely to atmospheric pressure which it only produces in part.

Structure of the heart.—Believing that the structure of the heart must be understood before its action can be explained, I undertook to examine it at a time when I was not aware of the exact state of knowledge on the subject. After several experiments, I found that the sheep's heart, boiled slowly in a solution of pearl-ash, answered my purposes very well. As there is still further room for investigation on the subject, I shall give a brief account of the manner in which I attempted to unfold the heart.

On raising a few fibres on the external surface of the heart, I found it easy to detach them from the substance of the ventricles, tracing them from the base towards the apex. I commenced on the surface of the right ventricle, the fibres are all spiral, running from right to left over the anterior surface, and back again from left to right on the posterior surface, until finally, after two or more turns, they cease at the apex; these are the external fibres, which on being raised let

into view those which are below them, and these, gradually, as we descend into them, are found to lie more transverse, but still spiral. Nothing like a laminated or fasciculated structure is observable, and the fibres lie very close to each other. The most superficial fibres extend to the apex, the most prominent part of which they constitute; those which are next beneath, extend also to the apex, into the composition of which they enter, and so on with others. The external fibres, at the very summit of the apex, are continued into the left ventricle, and there form the surface immediately under the lining membrane; those which I have spoken of as lying beneath the most superficial and external, hold the same relative position to the superficial internal, so that a certain portion of the fibres may be found composing the external and internal surfaces of the left ventricle, and these fibres, common to both surfaces, are the same and continued.

Continuing further to unfold the ventricles, it is found that the fibres fall short of the apex by degrees as we descend into the structure, until finally we find many of them confined to the composition of the base.

In the course of the unravelling, for such it may be fairly called, the anterior wall, which is composed of fibres to a great extent common to both ventricles, is removed. The right ventricle, however, is not composed entirely in this way; there are fibres on its internal surface which are common to it and the septum.

Having arrived at the substance of the left ventricle which is peculiarly its own, that is, after all the fibres which are common to the external part of both ventricles are removed but not broken, it is very easy to finish the unfolding, when it will be perceived that all the fibres intermediate to those which were first mentioned, after winding around twice or more then take a direction upwards, they enter into the composition of the *columnæ carneæ*.

We may consider most of the fibres as originating at the base of the heart wherever there is cartilaginous matter; but many of the fibres arise from the inner surface of the ventricles, and running out at the auriculo-ventricular opening become superficial and external after having given rounded margins to the cavities from which they passed; on the surface they run the course which was first described.

The termination of the fibres may also, in a great degree, be traced to small tendons, or cartilage looking into the cavities, or placed at the summit of the septum, where also the auricles are attached.

As the superficial fibres often wind around the ventricles two or three times before their spiral course terminates in that which is straight in the *columnæ carneæ*; so indeed do all the fibres beneath them. Suppose a fasciculus of these fibres to extend the course I

have pointed out, then it must be seen that it may be represented by a coil of wire, the inner part of which is continued in a straight direction, and at right angles with the coil; by depressing the extremity of this wire, which represents the continuation of the fasciculus in the columnæ carneæ, the coil will be converted into a conical spire, perpendicular to which the straight part of the wire still remains. Suppose another coil, smaller in diameter and similarly formed, be introduced within the first coil, and its straight wire be depressed somewhat less than the first, this will represent the next order of distribution, and so on. Attending to what was said in the general description, and retaining this mode of illustration in mind, it may be perceived that the ventricles are composed of muscular coils, disposed in spiral and somewhat conical form, that the spires become shorter as they are less superficial. But as they become less superficial, it must be remembered that the inner straight continued part of the coils become less perpendicular, and finally, in the most deep-seated, it forms a very acute angle instead of the perpendicular before described.

I have not yet obtained results in my experiments on the auricles so satisfactory as to induce me to attempt a description of them. This however is evident to me, that the fibres of the auricles and ventricles are perfectly distinct from one another. In one case out of seventeen I found a strong fasciculus of the fibres of the right auricle perfectly identified with the structure of the ventricles: the connexion between them was made by a distinct column about three-quarters of an inch long.

We often perceive muscular columns or bands extending from one side of a ventricle to the other; I have observed that their direction is always oblique, and not directly transverse from one side of the cavity to the other, under which circumstance they would greatly embarrass the diastole of the heart. These bands do not make it more difficult to examine the structure of the heart, as might be at first sight supposed. Even the net-work, which the ventricles sometimes present on their inner surface, will yield satisfactorily if the heart be properly prepared.

I am not prepared to offer an opinion as to the manner in which the diastole of the heart is produced. I would therefore merely ask: is it the consequence of muscular contraction? The only way in which I can perceive that the diastole of the ventricles may arise from muscular contraction, is that all the fibres of the heart are stimulated to contract at the same time, but we know that the internal surface of the heart is more irritable than the external, and this can be proved satisfactorily. It is the muscular fibres of the internal sur-

face, then, that are more irritable than the external surface; but it has been shown that there is no interruption or separation of continuity between the external and the internal muscular fibres. It appears from this that one part of a muscular fibre may be more irritable than another part. Now, if this be true, it may also be true, that one part of a muscular fibre can contract quicker than another part. Let us then suppose that the columnæ carneæ, which constitute but a small portion of the entire length of the fibres of the heart, is more irritable, and contracts more readily than the spiral part, would the ventricles on this account be thrown open suddenly, and closed a little more slowly, by the more gradual and superior power of the spiral fibres?

I have been to a considerable extent anticipated in my views of the structure of the heart by LOWER, who published his account of it in 1708.

Influence of respiration and of the active resiliency of the lungs in drawing blood into the thorax.—The experiments of Dr. Barry leave but little doubt I think, that inspiration, under some circumstances, tends to draw blood into the thorax; but I am induced to believe that he has greatly overrated the results. My object at present is not to analyze Dr. B's views, but to attempt to show in a few words, how far we are to regard atmospheric pressure and the expansion of the thorax, as calculated to influence the motion of the blood in the veins.

Speaking of the powers which circulate the blood in the veins, Dr. B. asserts, "of these powers the pressure of the atmosphere is by far the most *intense* in its degree, the most *constant* in its influence, and the most *unvarying* in its amount. It is that without which the circulation could not be maintained beyond a few moments." We shall presently see how Dr. Barry has attempted to verify this assertion, with but little success, and how the slightest evidences in its favour have been regarded by him as conclusive, while the want of them never, for a moment, elicited from him an expression of doubt as to the correctness of his doctrine, in the full extent to which he has urged it.

As it at once opens to view the ground-work of Dr. Barry's theory, I quote the following part of his "argument drawn from anatomy." "When the chest is enlarged by the act of *inspiration*, air rushes in through the trachea, to distend the air-cells, and force them to occupy that space in which the expanding parietes of the thorax tend to leave a vacuum. But as it is evident, that the air would follow the expanding sides of the chest much more readily if there were no cells to be distended, and as it is an unalterable law that all liquids in communication with an enlarging cavity, will be pressed towards it

if exposed at the same time to atmospheric influence, it became presumable that blood would be forced into the thorax through the cavæ during inspiration."

I make no objection to this argument; it is well known to every anatomist how powerfully elastic the substance of the lungs is, consequently the tendency which it constantly has to collapse, and to oppose a distending agent. When the thorax is expanding in inspiration, it is certainly tending to the production of a vacuum within, and the air rushing in through the trachea and its various ramifications to fill the vacuum as fast as it is formed, meets a resistance in the collapsing power of the lungs; now, as the power of resistance in the lungs is something opposed to the expanding power of the thorax on the one side, and to the pressure of the atmosphere on the other, we at once perceive strong reason to suspect, that all the veins communicating with the cavity of the thorax, and exposed to the influence of atmospheric pressure from without, are liable to have the blood which they contain pressed into the great venous trunks and the auricles of the heart when they dilate.

Dr. Barry's experiments to prove the production of a tendency to a vacuum in the chest during inspiration, I must make known in fewer words than he uses. 1st Experiment. A horse was thrown upon his right side, left jugular vein exposed, tied below its middle, opened an inch below the ligature, a large flexible catheter introduced into the open vein and directed towards the heart. To the other extremity of the catheter a spiral glass tube was connected, the extremity introduced into a vessel containing coloured fluid which ascended through the tube into the vein, and probably into the heart, during inspiration. Blood regurgitated into the tube when the respiration was hurried, but returned into the vein at the next inspiration. This I call a mere imitation of what we observe in the jugular veins of mammalia, and sometimes in the jugular veins of persons in coughing, laughing, &c. where the blood is forced into the veins of the neck, and drawn back again when inspiration is made. In the report made to the "Royal Academy of Sciences, Paris," upon "Dr. Barry's Memoir," by Baron CUVIER and DUMERIL, the observations of RUDIGER, SANTORINI, HALLER, VALSALVA, MORGAGNI and M. MAGENDIE, on the progression of the blood through the great veins towards the heart during inspiration are alluded to, not one of whom it appears accounts for this phenomenon correctly. To Dr. Barry then belongs the credit of having pointed out the mechanism by which it is effected, which will be further seen by a short notice of his second experiment. 2d Experiment. Two tubes ingeniously contrived

introduced into the cavity of the thorax, one on each side of the posterior extremity of the sternum, the animal being on its back. The other extremities were immersed in coloured liquid. The tubes penetrating into the cavities of the pleuræ made a communication between them and the external fluid. The fluid rose rapidly in the tubes during inspiration, and was drawn into the chest. I may also now give a brief account of Dr. B's 3d experiment. This was made with a view to ascertain whether a tendency to a vacuum was also produced in the bag of the pericardium. Having succeeded in introducing a tube into this bag, he proved that a fluid ascended through it during inspiration. These experiments leave no doubt as to the production of a tendency to a vacuum in the cavities now alluded to; but while this is admitted as true, it must be acknowledged that the power which they exert in drawing to them blood from the veins is a subject which still requires to be investigated. Dr. Barry lays an estimate on it which no person conversant with anatomy and the physical sciences will receive as correct, and while the Baron Cuvier and professor Dumeril in reporting on his memoir, speak in the highest terms of his experiments, they evidently have not included the opinions of Dr. Barry in their commendation.

In noticing the active resiliency of the lungs, let it not be supposed that I am giving my support to Dr. CARSON's doctrine. I believe in the active resilience of the lungs, and so does every person who is at all conversant with anatomy, but I give but little credit to the fine-spun doctrine of Dr. Carson.

As the lungs are highly elastic and in a forced state of distention while they remain sound during the life of an animal, so they have a constant tendency to collapse, but in order to do so, they should leave a vacuum in the cavities of the pleuræ, and also I think in the bag of the pericardium. It is very evident they have not the power to do so, and they must be regarded therefore at least in the quiescent state of the thorax, as leaving no empty space between them and the walls of the thorax. The collapsing power of the lungs is considerable, but no proper estimate has been made of it; we know how forcibly it draws up the diaphragm; it draws, we have reason to believe with equal power on the hard thoracic walls all around. Now as there are two great cavities separated from one another by the mediastinum, it is perfectly in accordance with experience to suppose, that the collapsing power of the lungs acts with as much power on the mediastinum as on any other surface with which the lungs are in contact. But the heart and pericardium are to a great extent concerned in opposing the collapsing power of the lungs; the pericardium is also a

sack, and its capacity is not filled by the heart, Dr. Carson therefore supposes that the collapsing power of the lungs keeps the pericardium in a *constant state of extension*, and that an antagonizing power to the muscular fibres of the heart is thus produced, which operates constantly to dilate the heart; I certainly cannot agree with the learned author of this doctrine, and think he has betrayed great deficiency of knowledge in pneumatics in forming it. Had he been better acquainted with this branch of philosophy, his doctrine would doubtless never have appeared in print. But, let us see in what more particularly his error consists. The collapsing power of the lungs is, as I have already said, considerable. Dr. Carson made experiments to ascertain its amount, and found that it was equal to a column of water seven inches high, its ratio then to atmospheric pressure is nearly as 1 to 56; its tendency to produce a vacuum is also in the same ratio, it cannot therefore with so great a power against it, form a vacuum.

I have not made experiments expressly with a view to measure the collapsing power of the lungs in any class of animals, but from general observation on the subject, I think the following opinions correct. The collapsing power of the lungs increases in an unknown ratio as the lungs are inflated from the state of entire collapse, to the size which they ordinarily occupy in any animal; beyond this their power of resisting a distending power increases in a far greater ratio than that before alluded to. The air rushing into the lungs while the thorax is expanding, distends the lungs and makes them keep pace with the expanding thorax, but when the air does not pass through the rima glottidis in volume commensurate with the increase of capacity produced in the thorax, then several things are liable to happen; there must be left a vacuum in the cavity of the pleuræ, or the diaphragm must be caused to move upwards to supply the deficiency of air; or blood in the veins, external to the thorax, must move in, or the air in the lungs must expand by its elasticity and dilate the lungs. If a very sudden and powerful effort be made to draw in air, it is probable, I think, that all of the preceding effects, except the first, would follow at the same time, but no one or two would subside before the others, they would all be terminated when any one of them restored the equilibrium of atmospheric pressure. Further, the effect which required the least power to produce it, would precede all the others in its extent. Here we want certain data, but we may form an opinion as to the forces concerned. Suppose the resilience of the lungs in such a case to be fifty; expansive power of the air three hundred; the latter then would prevail, and

prevent the production of a vacuum from the very commencement. The air thus rarified would however only resist external pressure, let us suppose as three hundred to three hundred and ninety. The diaphragm would, therefore, be pressed up into the thorax in an arched form, the blood would likewise be obliged to rush in, and collect in the cavæ, so that we should have all these effects produced at once, but not a vacuum. Now, suppose a slow inspiration be made, the effects, I think, shall be somewhat different from the preceding; the entrance of the air into the lungs shall expand them as freely and as quickly as the thorax would expand; the diaphragm instead of being pressed up, would be forced down, an evidence that the lungs under the last condition fill the cavities tightly from the commencement to the end of ordinary inspiration. I see no reason then for supposing that during ordinary inspiration, blood is moved into the thorax by atmospheric pressure. Nor has Dr. Barry ever proved that it does. Read his own words. "The connexion between the motions of the liquid in the tube and respiration cannot be satisfactorily observed while the horse is standing, because his breathing when in the erect posture, and at rest, is scarcely, if at all, perceptible." The circulation, however, we must presume, is going on rapidly in every part of the body, and according to Dr. Barry, even when a gum elastic catheter is introduced through one of the jugular veins, and passed almost as far as the superior cava, and through which no water will ascend, still Dr. Barry asserts, that atmospheric pressure is more powerful than any, or all the other forces in moving the blood through the veins.

"Here it is essential to remark, that if the communicating tube be introduced into the femoral vein of a dog or horse, and pushed no further towards the heart, inspiration will produce no effect upon the liquid in the cup, because the relative vacuum of the thorax can be filled up from the other veins of the animal's body." I will merely quote another passage from the report of the Baron Cuvier and Dumeril on his memoir.

"Your commissioners, however, must not conceal that in their *particular* opinion the act of inspiration which *appears* to produce a vacuum within the thoracic cavities of animals having lungs, is not sufficient to explain the motion of the blood in the veins of fishes, and of some reptiles in which the mode of respiration is different."

Let any vein, or number of veins be tied as near the heart as is possible, it will be found that on puncturing them, a stream of blood will issue and continue to flow perhaps as long as there is life in the

animal; or if they be not punctured, it will be found that they become full and greatly distended below the ligature, showing conclusively that atmospheric pressure is not the great cause of the return of the blood from all parts of the system. Even Dr. Carson admitted the strength of this objection against the extent to which he felt disposed to urge his doctrine. To produce additional proof on this point I deem unnecessary. It may be found in Harvey's valuable work, "De Motu Sanguinis;" Haller's Physiology; M. Magendie's Physiology; Bichat's Anatomie Générale, and indeed in almost every modern work. I can also refer with great pleasure to Arnott's Elements of Physics, second American edition.

I am now constrained to conclude this paper, in which my sole object has been to show the necessity of further investigations respecting the forces by which the blood is moved, and to expose the errors of those, as they have appeared to me, who, in their zeal for the advancement of medical science, have put forth doctrine upon doctrine, hypothesis upon hypothesis, error upon error.

I cannot but avail myself of the present opportunity to express my unfeigned thanks to Dr. HORNER, Professor of Anatomy in the University of Pennsylvania, who has warmly encouraged and aided me in my investigations, and directed my attention to various sources of information in my inquiry into the state of our knowledge on the subjects of this paper. To Dr. ISAAC HAYS, the Editor of the American Journal of the Medical Sciences, I am bound to make acknowledgments for the polite attentions which I have received from him, in directing my attention to numerous valuable works: but in particular, to Dr. SAMUEL JACKSON, Professor of the Institutes of Medicine in the University of Pennsylvania, my preceptor and sincere friend, under whose auspices I have pursued my studies, and to whom, more than to any other person, I am indebted for all that I regard valuable of what I have acquired in the study of medicine, I avail myself of this opportunity to express my gratitude and sense of his exalted talents, learning, and above all, of his goodness of heart.

Philadelphia, April 3d, 1833.

